

the behavior of this species, more commonly known as *C. neomexicanus* Lowe & Zweifel, 1952, so I feel qualified to support the application. The name *perplexus* has not been used by researchers in recent years and, given the controversy surrounding its historical use, it should be officially suppressed.

I earnestly and enthusiastically support the authors's proposal to place *neomexicanus* on the Official List and *perplexus* on the Index. I thank the Commission for their help in this very important nomenclatural matter.

(8) Support for the application has also been received from Prof Robert C. Stebbins (*Museum of Vertebrate Zoology, University of California, Berkeley, California 94720, U.S.A.*), Prof James L. Christiansen (*Department of Biology, Drake University, Olin Hall, Des Moines, Iowa 50311, U.S.A.*), Prof Roger Conant (*Department of Biology, 167 Castetter Hall, The University of New Mexico, Albuquerque, New Mexico 87131 1091, U.S.A.*) and Dr Joseph T. Collins (*The Center for North American Amphibians and Reptiles, 1502 Medinah Circle, Lawrence, Kansas 66047, U.S.A.*).

Comment on the proposed conservation of usage of 15 mammal specific names based on wild species which are antedated by or contemporary with those based on domestic animals

(Case 3010; see BZN 53: 28–37, 125, 192–200, 286–288; 54: 119 129, 189)

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I would like to express my strong support for the application by A. Gentry, J. Clutton-Brock and C.P. Groves, published in BZN 53: 28–37 (March 1996). My views are based on more than 30 years of experience as a vertebrate ecologist and wildlife conservation biologist conducting research into the ecology of domestication (Brisbin, 1974) and the conservation biology of the wild ancestors of domestic animals (Brisbin, 1995, 1996) and unique forms of feral (domestic returned to the wild state) wildlife (Brisbin, 1989, 1990; and Brisbin et al., 1994). Considering my background, I believe that I disprove the earlier claim of Schodde (BZN 54: 123; June 1997) that support for Case 3010 'comes largely from a relatively small group of archaeozoologists', and that the proposal brings 'confusing complications' to the 'very large world of ecologists, conservation biologists and wildlife managers'. On the contrary, as a person who works extensively in these three latter fields, I welcome this proposal and the clarification/simplification that it brings to my work. My position in this regard also serves to disprove a similar claim by Bock (BZN 54: 125) that this proposal would have the 'potential for creating considerable confusion ... for the large number of ecologists, wildlife biologists and conservationists dealing with these species'.

My work in these areas focuses particularly on the wild ancestors and feral counterparts of the domestic dog (Brisbin et al., 1994; Brisbin & Risch, 1997), pig (Brisbin, 1990; Mayer & Brisbin, 1991; and Oliver & Brisbin, 1993) and chicken (Brisbin, 1996; Peterson & Brisbin, in press). This work has been supported by a Financial Assistance Award between the U.S. Department of Energy and the University of Georgia (DE-FC09-96SR18546) and will form the basis for most of my comments concerning this proposal.

Much of my work has related to documenting the unique components of global biodiversity that are embodied in populations of the surviving genetically pure wild ancestors, primitive domestic breeds and long-term feral forms of species such as those named above. It is with regard to mustering support for the conservation of these underappreciated and often unrecognized components of biodiversity that the adoption of Case 3010 becomes a particularly critical issue. As pointed out by Gentry, Clutton-Brock & Groves (BZN 54: 127–129; June 1997), failure to adopt this proposal will have a considerably negative impact upon those of us who are trying to make a case for the conservation of these elements of biodiversity. The case to defend this biodiversity must be made to national and international regulatory bodies which act under the mandates of legislation and international agreements such as the United States' Endangered Species Act, the Convention on International Trade in Endangered Species of Wild Flora and Fauna (CITES), etc. Consider for example the negative impacts on public relations efforts, not to mention legal hindrances, that would be associated with a nomenclatural designation indicating that remnant free-ranging populations of wild wolves are really (taxonomically speaking) nothing more than domestic dogs (*Canis familiaris*)!

Since the best available evidence suggests that the domestic dog and wild wolf are indeed one species, designating both as *Canis lupus* could still solve the above problem. However, this has the consequence of producing confusion and problems for a number of issues such as law enforcement against the keeping of wolf hybrids by members of the public, and legal mandates in Australia for control of dingoes. I personally prefer the solution suggested by Wolsan (BZN 54: 189; September 1997), who advocated 'excluding domesticated forms from their ancestral wild species and labeling them with distinct names of full specific rank based on domestic animals'. My work with the ecology of the domestication of the species noted above strongly supports the scientific validity of this proposal. Such a separate specific designation would go a long way towards eliminating the present potential for confusion associated with taxonomic citations in scientific literature where the derivation of the particular animals used in the research is not otherwise indicated. For example, a study which describes the aggressive behavior of '*Canis familiaris*' towards humans under certain conditions would be interpreted quite differently if the subject involved was a wolf as opposed to some breed of domestic dog. The importance of such distinctions to ethologists has been clearly described by Mungall (BZN 54: 120–121; June 1997) in her commentary in support of Case 3010. I strongly support the assignment of separate taxonomic designations for domestic vs. wild ancestral forms of the same species as suggested by Gardner (BZN 54: 125–126; June 1997), who points out that 'domesticated mammals in most cases are reproductively isolated from their wild progenitors and warrant species status'. The problem is that in many cases there are still simply not enough data to say for sure whether a full species or

perhaps subspecies-level distinction is warranted between a given wild ancestor and certain of its related long-term feral and/or primitive domestic counterparts (Brisbin et al., 1994). As in other unsettled questions of taxonomic affiliation however, additional data, when collected in future studies, will hopefully clarify these relationships and thus settle disputes concerning such nomenclatural designations.

Schodde (BZN 54: 123–124; June 1994) has a good point when he raises the question as to just what taxonomic designation should be given to feral forms. I suggest that the answer to this question should be on a case-by-case basis and depend on the findings of long-term thorough studies of the archaeozoology, biogeography and ecology of the wild ancestor and the most primitive/long-term feral and domestic forms of the species in question. In cases of well-defined long-term primitive feral forms such as the Australian dingo / New Guinea singing dog (Brisbin et al., 1994) or the Andaman Island pig (Brisbin, 1990; Mayer & Brisbin, 1991; and Oliver & Brisbin, 1993), the case could well be made for a separate taxonomic designation at the species or subspecies level. This would largely depend, I would suggest, on the eventual determination of genetic distances using molecular genetic techniques. It should be pointed out however that Schodde (BZN 54: 121–122; June 1997) is not correct in stating that feral forms tend to reapproach wild ancestral stock in form through a long period of 'free out-breeding'. The longest-term feral pigs (those of the Andaman Islands and Ossabaw Island, Georgia, U.S.A.) have remained distinctive from wild boar in both external body form and cranial morphology (Mayer & Brisbin, 1991) and the longest-term feral dogs (the New Guinea singing dog and the Australian dingo) have in no ways come to resemble the body morphology of the wolf (Brisbin et al., 1994; Brisbin & Risch, 1997). In any case, this whole matter of feral animal taxonomy is certainly not resolved by opposing Case 3010. In fact, failing to support Case 3010 serves only to further muddy already troubled taxonomic waters.

Animal domestication has been one of the most important processes ever devised by humans to help promote their own survival and well-being across evolutionary time. The varied applications of this process with a variety of animal species have created an incredibly broad array of biodiversity, which is largely underappreciated by most conservation biologists. Many of the animal groups that embody the most unique and primitive expressions of this biodiversity are currently threatened with genetic extinction in the free-ranging state (Brisbin, 1995; Brisbin, 1996; and Peterson & Brisbin, in press). This problem has been exacerbated as human civilization and free-ranging/escaped modern domesticates continue to invade the ever-shrinking ranges of the last genetically pure remnants of the wild ancestors or long-term primitive feral counterparts of these forms. I urge the Commission to consider the potential negative impacts which a failure to support Case 3010 could have upon efforts to conserve and study these last unique remnants of the process that brought us our modern-day domestic animals and the dependence which we have upon them.

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